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The Notion of Member is the Heart of the Matter: On the Role of Membership Knowledge in Ethnomethodological Inquiry

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Abstract: In ethnomethodological inquiries, the tension between "subjectivity" and "objectivity" which is inherent in all qualitative social research, takes special meanings. In fact, those terms are rarely used in ethnomethodological research reports, or methodological writings. What is widely implied and often explicitly recognised, however, is that an ethnomethodologist has to "understand" the practices studied, before they can be analysed, and that this "understanding" involves the researcher using his or her "membership knowledge". In a way, this unavoidable use of membership knowledge for understanding what people are doing, is then turned from a implicit *resource* into an explicit *topic* for analysis. This can be illustrated by a consideration of the two research strategies for which ethnomethodology has become (ill-) famous, the "breaching experiments" initiated by its founder Harold GARFINKEL, and the use of recordings and transcripts of verbal interaction by ethno-methodology's most successful off-shoot, Conversation Analysis as initiated by Harvey SACKS. Varieties of a third strategy, ethnography, including the ethnography of specific (sub-) cultural practices, of technology use, and auto-ethnography, will also be discussed for its treatment of membership knowledge as resource and topic.

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1. Introduction[1]

In current qualitative research, the research process and its products are seen as inherently and unavoidably dependent on the "subjectivity" of the researcher, that is his or her "personal" characteristics and experiences. Furthermore, reflecting on this subjectivity, often labelled "reflexivity", including a public acknowledgement of one's personal stake in the research products, is often presented as laudable, if not as a moral obligation. Ethnomethodology does not seem to participate in these current trends, without in general joining the contrastive party of objectivism. "Subjectivity" does not figure prominently in ethnomethodological writing, nor does "objectivity", while "reflexivity" is used in a particular ethnomethodological sense, not as a virtue but as an inherent property of human life. It seems to make sense, therefore, to explore how ethnomethodologists deal with the issues which elsewhere are covered by "subjectivity" and "reflexivity". My argument will be that ethnomethodology's apparent exception is a consequence of its particular conception of the role of knowledge in social life. In line with its SCHÜTZian heritage, ethnomethodology stresses the fact that persons, as members of society, use and rely on a corpus of practical knowledge which they assume is shared at least in part with others. This "use and reliance" is mostly tacit, "seen but unnoticed". "Membership knowledge" is commonly treated as a self-evident "resource" rather than as an explicit "topic". For ethnomethodology, then, membership knowledge is the key issue in any discussion of its topic, but also a crucial aspect of its own methodology. Ethnomethodologists are themselves also, and unavoidably, *members*. Therefore, I will, in this article, present a general discussion of the ways in which ethnomethodological research is carried out, with special attention to the role of membership knowledge. I will start with an overview of what ethnomethodology is all about, including an explication of the sense of the terms "member" and "membership". Next, I shall consider some of the various strategies used in ethnomethodological research, including the so-called breaching experiments, the analysis of tapes and transcripts, and ethnography, in terms of their reliance on membership knowledge. In the final section, I will return to a discussion of the notions of "subjectivity" and "reflexivity", as used in qualitative social research at large, in the light of my observations on ethnomethodological research practices.

[1] I have, in this text, freely used adapted fragments of earlier as well as current writings, including TEN HAVE (1990) and (1999); see TEN HAVE (2004) for further elaborations.

2. What is Ethnomethodology

As a first approximation,[2] one can say that "ethnomethodology" (EM) is a special kind of social inquiry, dedicated to explicating the ways in which collectivity members create and maintain a sense of order and intelligibility in social life. It has emerged as a distinctive perspective and style of social research in the teachings and publications of one man, Harold GARFINKEL. From a varied set of "sources of inspiration", including on the one hand most prominently his teacher and PhD supervisor Talcott PARSONS, and on other the phenomenological philosophies of Alfred SCHUTZ, Aron GURWITSCH and Edmund HUSSERL, he has forged a new vision of what social inquiry could be. Taking off from PARSONS' synthesization of various classical traditions of sociological theorising, one can say that in ethnomethodology these have been "turned on their heads". For the DURKHEIMian strand in classical sociology, and social research more generally, the ultimate goal is to investigate "social facts", and their determinants, where "social facts" have the twin characteristic of being both "external" and "constraining" to the actions of individuals. In ethnomethodology, on the other hand – to adapt a phrase from Melvin POLLNER (1974, p. 27) – "facts are treated as accomplishments", that is, they are seen as being produced in and through members' practical activities.

In other words, while classical (DURKHEIMian) sociology is in the business of *explaining* social facts, the effort of ethnomethodology is directed towards an *explication* of their constitution. In his *Le suicide: Étude de sociologie*, Emile DURKHEIM tried to explain variations in suicide rates in terms of variations in kinds of social integration. An ethnomethodologist, however, might investigate the ways in which cases of sudden death get constituted as being "suicides", or, at a different level, how statistical information about various "rates" is used to construct a sociological explanation of suicide in terms of social "causes".[3]

For sociology, and social research in general, the interest in the factual status of "social facts" is limited to technical and practical issues of getting those facts right, in a methodologically sound way, and at reasonable costs. There is, for instance, an enormous methodological literature on designing, implementing and analysing social surveys. This literature is focussed on methodological choices that should guarantee a sufficient level of representative-

[2] My characterisation of ethnomethodology is a personal and selective one. Among the many other sources, I would specifically suggest to consult HERITAGE (1984) for a broad scholarly overview, SHARROCK and ANDERSON (1986) for a concise and sharp discussion of basic issues, BUTTON (1991) for a collection of essays dealing with ethnomethodological ways of treating some of the classic themes of the human sciences, and LYNCH (1993) for pointed and polemical discussions confronting ethnomethodology and the sociology of scientific knowledge.

[3] GARFINKEL (1967a, pp. 11-18; 1967b), see also ATKINSON (1978) for some early efforts in these directions.

ness, validity and reliability, and on practical problems of avoiding sampling error, non-response, interviewer influences on answering behaviour, misunderstandings between interviewers and respondents, etc. For ethnomethodology, survey design and analysis, and survey interviewing, are interesting as possible topics for study as are other kinds of accountable professional practice. The stance taken in such an investigation would not be one of "methodological" or "practical" interest, but rather "disinterested" as to the purpose or the practices studied, a stance which has been called "ethnomethodological indifference" (GARFINKEL & SACKS 1970, p. 345). In other words, ethnomethodology might be interested in studying survey-related practices as such, as exemplary ways in which the factual status of "social facts" is being established for the practical purpose of doing a survey.[4] In a similar way, one can find ethnomethodological studies of various practical activities that constitute qualitative research, such as "open-ended interviewing" or "ethnographic reporting".[5]

Ethnomethodology's relationship with its "mother discipline" sociology, and by extension to all "social science", is then rather ambiguous. Both share a deep interest in problems of social order and try to elucidate the organisation of social life in all its manifestations. But their general approach is tangential to one another. I would like to stress that this "tangentiality" should not in the first place be seen as a difference in "research methods", as ordinarily conceived, but, as stated above, as one of "interests", "problematics" or "conception". Rather than focussing on issues like the choice between qualitative and quantitative research, the problem is one of research purpose, or the functions various methods are having in the argumentation of a research project. Indeed, the observation that ethnomethodological enquiries have a "qualitative" character, does not produce, by itself, a commonality of analytic interests with other kinds of qualitative social research.

3. Some Core Concepts in Ethnomethodology

It may be useful, at this point, to discuss some characteristic analytic notions that have been used in GARFINKEL's most prominent ethnomethodological studies.

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[4] Cf. BENSON and HUGHES (1991) for an ethnomethodological consideration of survey research logic, LYNCH (2002) for an explication of ethnomethodological as contrasted with survey research interests, and HOUTKOOP-STEENSTRA (1995, 2000), and various contributions to MAYNARD, HOUTKOOP-STEENSTRA, SCHAEFFER and VAN DER ZOUWEN (2002) for studies of survey interviewing.

[5] Cf. BAKER (1997), MAZELAND (1992), MAZELAND and TEN HAVE (1996), ROULSTON (2001), RAPLEY (2001) on qualitative interviews, TEN HAVE (2001) on ethnographic reporting.

3.1 Accountability and reflexivity

In the first two pages of his Preface to the *Studies in ethnomethodology*, GARFINKEL has given a very dense characterisation of his program. Here is one crucial passage:

> "Ethnomethodological studies analyze everyday activities as members' methods for making those same activities visibly-rational-and-reportable-for-all-practical-purposes, i.e., 'accountable,' as organizations of commonplace everyday activities. The reflexivity of that phenomenon is a singular feature of practical actions, of practical circumstances, of common sense knowledge of social structures, and of practical sociological reasoning. By permitting us to locate and examine their occurrence the reflexivity of that phenomenon establishes their study." (GARFINKEL 1967a, p. VII)

The two core notions provided here are "accountability" and "reflexivity" and it should be noted right away that these terms get a rather special meaning in GARFINKEL's hands. While "accountability" in ordinary talk is often associated with liability, here it is closer to intelligibility or explainability, in the sense that actors are supposed to design their actions in such a way that their sense is clear right away or at least explicable on demand. People who stand in line for a service point, for example, show that they are doing just that by the way they position their bodies, but they are also able to understand and answer a question like "Are you standing in line?" or "Are you in the queue?" So the understandability and expressability of an activity as a sensible action is, at the same time, an essential part of that action. GARFINKEL uses "reflexivity" to focus on that "incarnate" property, as in the following quote from the start of his explication of ethnomethodology.

> "The following studies seek to treat practical activities, practical circumstances, and practical sociological reasoning as topics of empirical studies, and by paying to the most commonplace activities of daily life the attention usually accorded extraordinary events, seek to learn about them as phenomena in their own right. Their central recommendation is that the activities whereby members produce and manage settings of organized everyday affairs are identical with members' procedures for making those settings 'account-able.' The 'reflexive,' or 'incarnate' character of accounting practices and accounts makes up the crux of that recommendation." (GARFINKEL 1967a, p. 1)

Over the last few decades, the concept of "reflexivity", which basically just denotes an object's relation to itself, has mostly been used in the social sciences in the sense of a call to a self-conscious view of social science's activities. Such a moral-political appeal should be clearly distinguished from GARFINKEL's use of the term (cf. LYNCH 2000; MACBETH 2001). I will return to this difference in the final section of this paper.

3.2 Indexicality

Over the course of his successive publications, GARFINKEL has used a number of terms to denote local, time-bound and situational aspects of action. Prominent in the early work was "indexical", as in "indexical expressions", or when discussed as a property: "indexicality". Indexical expressions are, in principle those whose sense depends on the local circumstances in which they are uttered and/or those to which they apply. Expressions like "you" or "yesterday" are obvious examples. But then, if you think of it, on all occasions, all expressions (and actions) are in fact indexical. GARFINKEL writes about "the unsatisfied programmatic distinction between and substitutability of objective for indexical expressions" (GARFINKEL 1967a, pp. 4-7).

> "Features of indexical expressions motivate endless methodological studies directed to their remedy. Indeed, attempts to rid the practices of a science of these nuisances lends to each science its distinctive character of preoccupation and productivity with methodological issues. Research practitioners' studies of practical activities of a science, whatever their science, afford them endless occasions to deal rigorously with indexical expressions.
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> [...]
>
> Nevertheless, *wherever practical actions are topics of study* the promised distinction and substitutability of objective for indexical expressions remains programmatic in every particular case and in every actual occasion in which the distinction or substitutability must be demonstrated. In every actual case without exception, conditions will be cited that a competent investigator will be required to recognize, such that in that particular case the terms of the demonstration can be relaxed and nevertheless the demonstration be counted an adequate one." (GARFINKEL 1967a, p. 6)

In other words, bridging the gulf between on the one hand abstract notions, as expressed in so-called objective (that is context-free) expressions, and on the other hand concrete instances which are inevitably tied to local circumstances and contexts, is an endless task. This task is always cut-off before it is completely finished, that is, as soon as the practical circumstances demand and allow a solution which is "good enough" for the purpose at hand. Indexical expressions are the preferred means for such solutions and are therefore the chosen topic for ethnomethodological investigations.

> "The properties of indexical expressions and indexical actions are ordered properties. These consist of organizationally demonstrable sense, or facticity, or methodic use, or agreement among 'cultural colleagues.' Their ordered properties consist of organizationally demonstrable rational properties of indexical expressions and indexical actions. Those ordered properties are ongoing achievements of the concerted commonplace activities of investigators. The demonstrable rationality of indexical expressions and indexical actions retains over the course of its managed production by members the character of ordinary, familiar, routinized practical circumstances.

[...]

I use the term 'ethnomethodology' to refer to the investigation of the rational properties of indexical expressions and other practical actions as contingent ongoing accomplishments of organized artful practices of everyday life." (GARFINKEL 1967a, p. 11)

So what we have as an "essential tension" in social life is that indexicality can never be fully "repaired" by substituting abstract and objective, supra-situational expressions, descriptions or instructions for inevitably "inexact" indexical expressions and acts. But, at the same time, practical actors always are able to "get by" in one way or another. Or, to borrow from a notion that came to be used later in GARFINKEL's writings (such as 1991), the philosophical problem of the gulf between the abstract and general on the one hand and the concrete and situational on the other, can, for ethnomethodological purposes, be *respecified* as a problem that members of society solve as a matter of course in their everyday activities.

3.3 The documentary method

This theme surfaces again and again in the later chapters of *Studies in ethnomethodology*. Chapter 3, for instance, discusses – and demonstrates – what GARFINKEL calls "the documentary method of interpretation", which he defines in the following way:

"The method consists of treating an actual appearance as 'the document of,' as 'pointing to,' as 'standing on behalf of' a presupposed underlying pattern. Not only is the underlying pattern derived from its individual documentary evidences, but the individual documentary evidences, in their turn, are interpreted on the basis of 'what is known' about the underlying pattern. Each is used to elaborate the other." (GARFINKEL 1967a, p. 78)

So here again we see a kind of two-layered model of social knowledge: the abstract layer of general knowledge, here "patterns", elsewhere "objective expressions", or in SCHUTZ' work "typifications", and the concrete level of actual instances, situated actions, here "documents", and elsewhere "indexical expressions". The always awaiting task, the "contingent ongoing accomplishment of organized artful practices of everyday life", is to connect the two, by giving accounts, by adding "etc. clauses" to statements, etc. (sic). It is that condition that is responsible, so to speak, for the "incarnate reflexivity" discussed before.

3.4 Some recent themes

A cluster of interrelated themes in GARFINKEL's later work have to be mentioned here. While some of his early writings could be read to suggest that ethnomethodology would be in the business of formulating general rules, statements, practices or procedures used in the constitution of local social orders,

the later work stresses the idea that those practices etc. are too intimately tied to the occasions at which they are being used to be discussed "independently" of them. This has been especially clear in ethnomethodological studies of a range of complicated professional activities, as in studies of research laboratories (LYNCH 1985 and many other publications), mathematical proofing (LIVINGSTON 1986) and piano improvisation (SUDNOW 1978, 2001). The general idea is that conventional studies of various specialised "trades" miss the essential "what" of those trades in favour of traditional sociological features like "professionalization", "status considerations", "lines of communication", etc. GARFINKEL has suggested that in order to be able to study the specifics – the "quiddity" or "just whatness" – that make up a particular trade, an investigator should develop a rather deep competence in that trade. This has been called the "unique adequacy requirement of methods" (GARFINKEL & WIEDER 1992). Still later GARFINKEL dropped the term "quiddity" or "just whatness" in favour of "haecceity" or "just thisness", presumably in order to avoid suggestions of a stable "core" that would define a particular practice. Whatever the fancy terms, the urge is still to study the rational, in the sense of reasonable, properties of indexical expressions and indexical actions (GARFINKEL & SACKS 1970). As already stated, the mission of recent ethnomethodology has been formulated a one of "respecification" of the classic concepts of western science and philosophy, such as "order", "logic", "rationality", "action", etc., *as* members' practices (cf. BUTTON 1991; LYNCH 1993; LYNCH & BOGEN 1996). In other words, the grand themes of our intellectual culture are taken up in a fresh way as embodied in local, situated and intelligible practices.

3.5 Membership

Both in my own text above, and in the various quotes from GARFINKEL, the concept of "member(s)" was used in places where others might have chosen "person(s)" or "individual(s)". This usage is a principled one. Ethnomethodology is not interested in "individuals" as such, but in the *competences* involved in being a bona-fide member of a collectivity. As GARFINKEL writes in a note:

> "I use the term 'competence' to mean the claim that a collectivity member is entitled to exercise that he is capable of managing his everyday affairs without interference. That members can take such claims for granted I refer to by speaking of a person as a 'bona-fide' collectivity member. (...) The terms 'collectivity' and 'collectivity member' are intended in strict accord with Talcott Parsons' usage in *The Social System* [...]" (GARFINKEL 1967a, p. 57, n. 8)

In their collaborative essay, Harold GARFINKEL and Harvey SACKS (1970) have elaborated this point as follows:

> "The notion of member is the heart of the matter. We do not use the term to refer to a person. It refers instead to mastery of natural language, which we understand in the following way.
>
> We offer the observation that persons, because of the fact that they are heard to be speaking a natural language, somehow are heard to be engaged in the objective production and objective display of commonsense knowledge of everyday activities as observable and reportable phenomena. We ask what it is about natural language that permits speakers and auditors to hear, and in other ways to witness, the objective production and objective display of commonsense knowledge, and of practical circumstances, practical actions, and practical sociological reasoning as well. What is it about natural language that makes these phenomena observable-reportable, that is account-able phenomena? For speakers and auditors the practices of natural language somehow exhibit these phenomena in the particulars of speaking and that these phenomena are exhibited is thereby itself made exhibitable in further description, remark, questions, and in other ways for the telling.
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> The interests of ethnomethodological research are directed to provide, through detailed analyses, that account-able phenomena are through and through practical accomplishments. We shall speak of 'the work' of that accomplishment in order to gain the emphasis for it of an ongoing course of action. The work is done as assemblages of practices whereby speakers in the situated particulars of speech mean something different from what they can say in just so many words, that is, as 'glossing practices.'" (GARFINKEL & SACKS 1970, p. 342)

In short, the notion of "member" refers to capacities or competencies that people have *as* members of society; capacities to speak, to know, to understand, to act in ways that are sensible in that society and in the situations in which they find themselves. The problem, then, with which I deal in this paper is how ethnomethodological studies use and depend upon the active use of membership knowledge in order to study "membership" as a phenomenon.

4. Ethnomethodology and Common Sense Procedures

Since ethnomethodology has an interest in the procedural study of common sense as it is used practically, it is faced with a peculiar methodological problem. This may be glossed as "the problem of the invisibility of common sense". Members have a practical rather than a theoretical interest in their constitutive work. They take common sense and its constitutive practices for granted, unless some sorts of "trouble" make attention necessary. So an early strategy of GARFINKEL was to "breach" expectations in order to generate this kind of trouble (GARFINKEL 1963, 1964; 1967a, pp. 35-75). For ethnomethodology, common sense practices are the *topic* of study, but those practices are also, unavoidably, used as a *resource* for any study one may try to undertake (c.f. ZIMMERMAN & POLLNER 1971). Without the use of common sense, its object of study would be simply unavailable, because it is constituted by the

application of common sense methods, such as "the documentary method of interpretation" (GARFINKEL 1967a, pp. 76-103). So the problem for ethnomethodology is how common sense practices and common sense knowledge can lose their status as an unexamined "resource", in order to be a "topic" for analysis. Formulated in this way, it is a double-faced problem: on the one hand a problem of minimising the unexamined use of common sense, and on the other that of maximising its examinability. This double-sided problem seems to be in principle unsolvable, one is bound to lose either the resource or the topic. So what one has to do is to find practical solutions, which are unavoidably compromises. I will presently suggest a typology of the solutions that have been tried in ethnomethodology so far.

The first strategy is especially prominent in GARFINKEL's early work (1963, 1964, 1967a). This strategy consists of the close study of sense-making activities in situations where they are especially prominent. Such situations are those in which sharp discrepancies, between on the one hand existing expectations and/or competencies, and on the other practical behavioural and/or interpretive tasks, necessitate extraordinary sense-making efforts by members. Such situations may occur naturally – as in the case of a "transsexual" studied by GARFINKEL (1967a, pp. 116-85) – or they may be created on purpose – as in the "breaching experiments", mentioned before.

In order to escape some of the practical and ethical problems generated by such experiments, a second strategy was developed. In this researchers study their own sense-making work by putting *themselves* in some kind of extraordinary situation. This may be a situation where routine sense-making procedures are bound to fail, or where one has to master a difficult and unknown task, or where one is instructed by a setting's members to see the world in a way that is natural for them but not for oneself. MEHAN and WOOD (1975) use the expression "becoming the phenomenon", while SCHWARTZ and JACOBS (1979) recommend strategies of becoming The Stranger or The Novice. Out of many possible examples I would like to mention David SUDNOW's (1978, 2001) study of becoming a jazz piano player, and Lawrence WIEDER's (1974) study of his being instructed in the use of "the Convict Code" as a general interpretive and explanatory device in a half-way house for paroled addicts. A special case of a "procedural self-study" is available in a book by Albert ROBILLARD, *Meaning of a Disability: The Lived Experience of Paralysis,* (1999), in which he describes his experiences as a disabled person suffering from the ALS (Amyotrophic Lateral Sclerosis) disease.

The third strategy is the one that most resembles traditional ethnographic fieldwork. It consists of closely observing situated activities in their natural settings and discussing them with the seasoned practitioners, in order to study the competences involved in the routine performance of these activities. To further this close study, or to be able to study these activities after the fact, recording equipment may be used, but quite often researchers using this strat-

egy rely on traditional note-taking in order to produce their data. Examples of this kind of study can be found in GARFINKEL's (1967a, pp. 104-15) work on juries and coroners, ZIMMERMAN's (1969) study of case-workers in a welfare agency, and LYNCH's (1985) research on laboratory scientists.

The forth strategy involves the study of ordinary practices by first mechanically recording some of their "products", by the use of audio or video equipment, as is the standard practice in Conversation Analysis (CA). These recordings are then transcribed in a way that limits the use of common sense procedures to hearing what is being said and noting how it has been said. The transcriptions are used to locate some "orderly products". It is the analyst's task, then, to formulate a "device" which may have been used to produce that "product" and phenomena like it (c.f. SACKS 1984).[6]

In actual practice, these strategies tend to be combined in various ways. In examples of the first three types, a tendency exists to use literal quotes from what was said by the research subjects, as in GARFINKEL's (1963, 1964, 1967a) reports of his "experiments", while in more recent studies recordings and transcripts tend to be used, as in GARFINKEL, LYNCH, and LIVINGSTON (1981) and LYNCH (1985). So a technical aspect of the fourth strategy is often adopted in the first three. WIEDER's study, here cited as exemplifying the second strategy can also be seen as an example of the third, as his analysis of his own learning of and being instructed in "seeing" the world of the halfway house in terms of "the code" is embedded in general ethnographic descriptions. There is a major difference, however, between the first three strategies – ethnomethodological studies in the stricter sense – and the fourth – CA, at least in its "pure" form. In the first set, specific circumstances are created or sought out, where sense-making activities are more prominent and consequently easier to be studied. In this way ethnomethodology displays a strategic preference for the extra-ordinary.[7] In contrast to this, pure CA tends to focus on the utterly mundane, the ordinary chit-chat of everyday life. While in ethnomethodology the "visibility problem" is – in part – solved by the creation or selection of "strange" environments, in CA this "estranging" task is performed by the recording machine and the transcription process. In more recent years, however, CA-type of analyses are increasingly embedded in and inspired by more ethnographically informed understandings, especially in so-called "workplace studies" focussed on technologically complex environments.[8]

The general idea lying behind the use of these strategies is thus to evade as far as possible the unthinking and unnoticed use of common sense that seems

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[6] Space forbids a more extensive discussion of this most successful branch of ethnomethodology; current overviews include HUTCHBY and WOOFFITT (1998) and TEN HAVE (1999).

[7] This seems less so for the third type. There is a tendency, though, for selecting settings in which fact-production is a major task, as in the examples quoted earlier.

[8] See BUTTON (1993), HEATH and LUFF (2000), LUFF, HINDMARSH, and HEATH (2000), SUCHMAN (1993) and a number of other publications by these authors.

to be inherent in empirical research practices in sociology. The ethnomethodological critique of these practices comes down to the objection that in so doing one studies idealised and de-contextualized "reconstructions" of social life, made by the research subjects and/or the researcher, instead of that life in its own situated particulars.[9] So ethnographers may be said to study their own field notes as an unexamined resource for their study of a community's life. Or researchers using interviews study the responses they have recorded as an unexamined resource for their study of "underlying" opinions and unobserved activities. In both cases, the situated "production" of those materials is not given systematic attention *in its own right*. The theoretical objects of such studies tends to be either individuals or collectivities. In contrast to such a "methodological individualism" or "collectivism", ethnomethodology and CA prefer a position that is closer to what Karin KNORR-CETINA (1981, 1988) has called "methodological situationalism".[10]

5. Common Sense as Inevitable Resource

The above critique concerning researchers' reliance on common sense, i.e. their membership knowledge, can also be turned against ethnomethodology and CA themselves. Although the "unthinking" use of such knowledge may be minimised, it cannot be eliminated completely, but this fact is not too often acknowledged. I will now present three cases where ethnomethodological writers have discussed this problem quite frankly. The first of these is Don ZIMMERMAN's "Preface" to WIEDER's (1974) half-way house study.

ZIMMERMAN points to the general, sensible and unavoidable use of what he calls "idealizations" in the natural and social sciences as well as in everyday life. Idealisations are selective, abstract and logically coherent constructions that are used to collect phenomena in terms of selected features judged to be relevant from a specific, for instance theoretical, point of view. Although he acknowledges the success of this procedure in the natural sciences, he sees certain drawbacks in its use in the social sciences: "a necessary consequence is the suppression of whole classes of data" (ZIMMERMAN 1974, p. 21). He specifically objects to the use of such idealisations that ignores the fact that idealisation is a feature of the social life studied itself.

> "Thus, ethnomethodologists would contend that these idealizations in the human sciences have ignored the fact that idealization occurs naturally within

[9] For that reason those analyses are called "constructive" (GARFINKEL & SACKS 1970); cf. the quotes from ZIMMERMAN below.

[10] She has formulated this position in terms of the then-current micro/macro and agency/structure debates: "I shall call methodological situationalism the principle which demands that descriptively adequate accounts of large-scale social phenomena be grounded in statements about actual social behaviour in concrete situations" (1988, p. 22).

> the domain of scientific theorizing (which is, after all, done from within the world) and takes place as well within the domain of everyday life – in the form of common-sense typifications [...]. For ethnomethodology then, 'idealization' (of either scientific or common-sense form) is a phenomenon for study, not a resource [...]. Though ethnomethodologists must themselves idealize their phenomena in some fashion when pursuing an analysis, their approach differs from current constructive theorizing in that their idealizations attempt to incorporate the view that, from the outset, societal members recognize and accomplish the orderly structures of their world [...] via the use of idealizations.

The phenomena of interest, then, are what Schutz (1962) refers to as second-order phenomena, namely members' idealizations of their own and others' behavior [...] social reality consists of the common-sense, practical activity of everyday 'idealizations' of the social world and activities within it [...]. For ethnomethodologists, idealizations (or rational constructions) of the social world must be recognized as also having the features of being 'done from within the world' and being 'part and parcel of that world', i.e., what Garfinkel (1967) calls 'reflexive features'." (ZIMMERMAN in WIEDER 1974, pp. 22-23)

So idealisations are always and unavoidably used, in ordinary life as well as in the sciences. The point is to recognise this and to take it into account in one's own idealising practices. How this is to be done is less clear, however.

My second case throws some light on this from a CA-inspired perspective. In a critique of "speech act theory" as proposed by J.L. AUSTIN, Roy TURNER writes:

> "As a solution to the vexed problem of the relation between the shared cultural knowledge (members' knowledge) that the sociologist possesses and the analytic apparatus that it is his responsibility to produce, I propose the following:
>
> A. The sociologist inevitably trades on his members' knowledge in recognizing the activities that participants to interaction are engaged in; for example, it is by virtue of my status as a competent member that I can recurrently locate in my transcripts instances of 'the same' activity. This is not to claim that members are infallible or that there is perfect agreement in recognizing any and every instances; it is only to claim that no resolution of problematic cases can be effected by resorting to procedures that are supposedly uncontaminated by members' knowledge. (Arbitrary resolutions, made for the sake of easing the problems of 'coding', are of course no resolution at all for the present enterprise.)
>
> B. The sociologist, having made his first-level decision on the basis of members' knowledge, must then pose as problematic how utterances come off as recognizable unit activities. This requires the sociologist to explicate the resources he shares with the participants in making sense of utterances in a stretch of talk. At every step of the way, inevitably, the sociologist will continue to employ his socialized competence, while continuing to make explicit what these resources are and how he employs them. I see no alternative to these procedures, except to pay no explicit attention to one's socialized knowledge while continuing to use it as an indispensible aid. In short, sociological

> discoveries are ineluctably discoveries from within the society." (TURNER 1971, p. 177)

What TURNER suggests is that ethnomethodological research is done in two phases. In the first the researcher uses his own membership knowledge to understand his materials, while in the second he analyses this understanding from a procedural perspective.[11] What TURNER does not mention, but what has become a standard in CA afterwards, is that the analyst can inspect subsequent utterances to see whether these display any specific understandings of previous utterances, either by other participants, or by the original speaker himself or herself (cf. SACKS, SCHEGLOFF, & JEFFERSON, 1978, p. 44).

The four strategy-types, discussed above, differ in the way in which they produce their materials. But always the study of these materials can be seen as organised in these two phases of membership understanding and procedural analysis. In WIEDER's (1974) book on a half-way house, for instance, the first part is largely devoted to an ethnographic study of the setting from which the concept of a Convict Code emerges, while the second deals with the ways in which this Code is used as a daily interpretive and explanatory device.

My third case of ethnomethodologists discussing their reliance on membership knowledge is taken from the book by Michael LYNCH and David BOGEN (1996) *The spectacle of history: speech, text, and memory at the Iran-Contra hearings*, which is the study of the ways in which the parties to these hearings struggle to have their version of "what happened" recorded as the facts of the case.

In the introduction they write that their aim is to describe "the production of history", and not to "deconstruct" it. In fact, a major phenomenon in those hearings was the pervasiveness of "deconstruction" as a practical activity, as each party tried to undermine the accounts provided by the other. Therefore, "deconstruction does not identify our own methodological agenda, but instead it is a perspicuous feature of the struggle we describe". And they continue:

> "We shall assume an ability to describe and exhibit recognizable features of the video text we have chosen to examine. In this effort we shall inevitably engage in constructive (i.e., productive) practices, such as using the video text as a proxy for the live performances of interrogators and witnesses, and selectively using written transcripts to exhibit recurrent discursive actions." (LYNCH & BOGEN 1996, p. 14)

In other words, they rely on their own ordinary members' competences as any (informed) viewer/ hearer of the tapes would, and they concede that their own use of tapes and documents inevitably also involves "constructive" work, which might be criticised as well by others.

> "Although it is commonplace in the social sciences to lay out a set of methodological procedures that provide reasonable foundations for the selection and interpretation of data, in this study we trust that readers will be able to

[11] A similar model for ethnomethodological research has been developed by Ilja MASO (1964).

> discern our methods by reference to what we say about the subject matter. Our methods are organized around, and take many of their initiatives from, the complexity and circumstances of the case at hand." (LYNCH & BOGEN 1996, p. 15)

So again, they present their own, ethnomethodological work on the data as "ordinary" and intelligible to "any member". And then they construct a contrast between this ordinary way of knowing with what are presented as ideals in conventional social science.

> "Although it is fashionable to attribute latent epistemologies to a text or practice being analyzed, ethnomethodology's approach to practical action and practical reasoning is more in line with the Aristotelian concept of 'phronesis.' Unlike episteme – the geometrical method of deducing proofs from axioms – phronesis takes its departure from the conventional recognizability of a perspicuous case. The presumption is that a community of readers will grasp enough of the details in question, with no need to justify such understanding on ultimate grounds, so that relevant maxims and precedents can be brought to bear on the case and extended to others like it. The failure of such a method to live up to the universal standards of procedure and proof associated with Euclidean geometry carries no necessary stigma. Indeed, it can be argued that science and mathematics do not fully exemplify episteme, and that at the moment of their production all inquiries involve an effort to come to terms with relevant circumstances." (LYNCH & BOGEN 1996, p. 15)

In effect, then, the authors offer a contrast between "ordinary" understanding practices and "formal" idealisations concerning proper ways of knowing, that are ascribed to mathematics and the sciences, although they suggest that even inquiries that fall under the latter auspices in actual fact also require "ordinary" practices of understanding (cf. for further elaborations and illustrations: LIVINGSTON 1986; LYNCH 1985, 1993). So, rather than claiming adherence to a set of formal principles, they, as ethnomethodologists, refer to their co-membership of a "community of readers" as a good enough basis for the intelligibility of their research materials as well as their own elaborations of those materials.

> "Ethnomethodology makes a topic of cases under inquiry in law, medicine, science, and daily life. This does not necessarily place the ethnomethodologist at a metaphysical or epistemological advantage vis-à-vis the practical actions studied, since any analysis of such actions is itself responsible for coming to terms with the circumstantially specific and immanently recognizable features of the case before it." (LYNCH & BOGEN 1996, p. 15)

They are not after some sort of "deeper" understanding of what happened and they do not try to replace one or another theory of meaning with their own. And neither are they trying to evaluate the truth value of one or another version of "what happened".

> "In view of the fact that so much social-scientific, literary, and philosophical effort has been devoted to getting to the bottom of discourse, our aim of sticking to the surface of the text may strike some readers as curious. It is our view,

> however, that any deeper readings would have to ignore the complexity and texture of the surface events, and thus they would fail to explicate how an order of activities is achieved as a contingent, moment-by-moment production." (LYNCH & BOGEN 1996, p. 16)

What should again be evident in these remarks is that ethnomethodology takes a very special position vis à vis commonsense knowledge and ways of knowing. They constitute an unavoidably used resource, but are also the topic of inquiry, to repeat what I have noted earlier referring to ZIMMERMAN and POLLNER (1971). We can note, moreover, two important consequences of this position. The first is that in the "first phase" of their inquiries, ethnomethodologists' reliance on commonsense methods of knowing puts them in a relation of cultural colleagues vis à vis their readers, and therefore they do not need any special warrants for their claims to understanding their materials. The second, however, connected to the second phase of inquiry, necessitates that they take a distance vis à vis the differential interests and disputes of commonsense life. So in the case of LYNCH and BOGEN, they are not in a position to take issue with the disputes they study, but rather they study the ways in which these differences are "produced" in the circumstances in which they occur. The label used to point to this particular kind of distantiation is "ethnomethodological indifference", which I will take up again in the last section of this article.

6. Using One's Membership Knowledge to Study Membership in Action

Although ethnomethodology has a single origin, Harold GARFINKEL, it does not have a single and unitary program. For instance, the methodological and epistemological distances between ethnomethodology à la GARFINKEL and conversation analysis (CA) seems in some respects so large, that many consider the latter to be a discipline of its own, related to ethnomethodology but still basically different. These and other differentiations are specially prominent when we consider the themes of this issue, "subjectivity" and "reflexivity".

6.1 Using recordings

In scanning programmatic, methodological, and substantive writings, one may find a variety of confessions, denials, or evasions of "subjectivity". I already noted a quite heavy reliance on recordings, transcriptions, and literal quotation. These can be seen as devices to *evade* subjectively accountable renderings of data. Harvey SACKS explicates his reasons for working with recordings as follows:

> "I started to work with tape-recorded conversations. Such materials had a single virtue, that I could replay them. I could transcribe them somewhat and study them extendedly – however long it might take. The tape-recorded materials constituted a 'good enough' record of what happened. Other things, to be sure, happened, but at least what was on the tape had happened. It was not from any large interest in language or from some theoretical formulation of what should be studied that I started with tape-recorded conversations, but simply because I could get my hands on it and I could study it again and again, and also, consequentially, because others could look at what I had studied and make of it what they could, if, for example, they wanted to be able to disagree with me." (SACKS 1984, p. 26)

So for him recordings had two important advantages, they allowed repeated study and they could be shared. Repeated study of a recording has an objectifying effect. It allows one to discover phenomena which would otherwise remain hidden in the fast flux of life, that is, recordings have an important heuristic or *analytic utility*. The possibility to share one's material with others adds to this objectification. It makes the evidence on which analytic claims are based accessible to others, and so enhances its *evidential utility*.[12] HERITAGE and ATKINSON present similar argument, but in even stronger "objectivist" terms:

> "[T]he use of recorded data serves as a control on the limitations and fallibilities of intuition and recollection; it exposes the observer to a wide range of interactional materials and circumstances and also provides some guarantee that analytic conclusions will not arise as artifacts of intuitive idiosyncracy, selective attention or recollection or experimental design. The availability of a taped record enables *repeated* and *detailed* examination of particular events in interaction and hence greatly enhances the range and precision of the observations that can be made. The use of such materials has the additional advantage of providing hearers and, to a lesser extent, readers of research reports with *direct* access to the data about which analytic claims are being made, thereby making them available for public scrutiny in a way that further minimizes the influence of individual preconception." (HERITAGE & ATKINSON 1984, p. 4)

Recordings, then, transform the recorded events into transportable "objects", or to use an expression coined by Bruno LATOUR (1987, p. 228) "immutable mobiles", which provide for its examinability and share-ability. In a sense then, conversation analysts take a "realist" or "objectivist" position vis à vis recordings. Transcriptions, on the other hand, are conceived in a different manner, while recordings are seen as the real data, transcriptions only offer a handy approximation. HUTCHBY and WOOFFITT (1998, p. 74), for instance, suggest to treat a transcript as a "representation", while the tape should provide a "reproduction" of the original event. HERITAGE and ATKINSON formulate the issue as follows:

> "[C]onversation analysts do *not* claim that the transcription system captures the details of a tape recording in all its particulars, *or* that a transcript should

[12] The idea that records and transcripts have both an "analytic utility" and an "evidential utility" has been discussed in these terms by ASHMORE and REED (2000).

> (or even could) be viewed as a literal representation of, or observationally adequate substitute for, the data under analysis. Like all transcription systems, the one used (in CA) is necessarily selective [...] and indeed this system is particularly concerned with capturing the sequential features of talk." (HERITAGE & ATKINSON 1984, p. 12)

Conversation analysts do use a more or less "standardized" transcription "system", developed over the years by Gail JEFFERSON (cf. JEFFERSON 1985, for example), but like any standardised system, it requires "individual" and "local" decisions for its application in practice. These ad hoc practices remain mostly hidden and are rarely if ever examined in detail.[13]

In a next phase, the analyst tries to "understand" what is happening on the tape and what the recorded utterances "mean" and "do" in their sequential context (TEN HAVE 1999, pp. 34-35). Tapes and transcript as-understood-by-the-researcher are then the "real data" for a CA analysis. As discussed by Roy TURNER in the previously given quotation, the analyst inevitably uses his or her membership knowledge to understand the transcript (and also to make sensible transcripts, of course). Within the CA research tradition, two special strategies have become established to try to correct possible idiosyncrasies in hearing/seeing and understanding. The first, relying on what is available in the data themselves, already noted before, consists of inspecting the data for explicit and implicit formulations or demonstrations of understandings by the participants in the recorded interactions themselves. The second relies on the data's share-ability indicated in the quotes from SACKS and HERITAGE and ATKINSON above. It is called a "data session" and consists of analysts coming together for a free discussion of some piece(s) of data, i.e. tape recordings and/or transcripts. In this context individual insights and intuitions can be exchanges and criticised freely under the auspices of the basic rule that any argument has to be made "in the presence of data", that is referring to the details of actual cases as available in the research materials at hand. In this way, one can try to promote an intersubjectively constituted understanding in an early phase of the research trajectory. In such a session, the tape functions as a "given object", while all subsequent re-workings of it – transcription, understanding and analysis – are open to intersubjective scrutiny.[14]

The CA convention to publish data excerpts (transcriptions) together with the analytic claims that are made on their basis, is another strategy to provide for a data-based discussion of CA analyses. CA, therefore, can be characterised as a research tradition in which "subjectivity", in the sense of subjective intui-

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[13] Making transcription is a practical activity and therefore inescapably has all the *ad hoc* properties that GARFINKEL (1967a) has discussed. For more extended discussions of recording and transcription, see TEN HAVE (1999, pp.47-98, and 2002) and ASHMORE and REED (2000).

[14] Cf. TEN HAVE (1999, pp.123-125) for a more elaborate discussion of data sessions, and especially JORDAN and HENDERSON (1990) for a description of one research setting's practices.

tions, is given a legitimate place in the early phases of the research.[15] In later phases, however, a double-faced discipline is supposed to be exercised. On the one hand there is an obligation to provide supportive data, and also to be open to seemingly contrary evidence, in the form of "deviant case analysis" (cf. TEN HAVE 1999, pp. 136-137 and *passim*). And on the other hand, there is an expectation to subject one's analytic insights and conclusions, together with the relevant data, to the critical considerations of one's analytic colleagues.

6.2 Detached observation

In those ethnomethodological studies that rely on *direct* observation, rather than mediated observation through recordings, the picture is rather different, and quite varied as well. Some reports of ethnographic studies from an ethnomethodological perspective are written in a rather detached manner, reporting on what there was to be seen and heard without taking the observer's "personal" involvement into account. David SUDNOW's (1967) study of hospital routines, *Passing on: the social organization of dying,* is a case in point. Although some of the scenes he described must have been quite dramatic, and he lets you feel the horror of some of the standard routines as well, the overall tone is detached, almost impersonal. In D. Lawrence WIEDER's (1974) study of a half-way house for paroled (ex-?) addicts, *Language and social reality: the case of telling the convict code*, the person of the researcher has a greater "presence" in the text. He reports how he interacted with staff and residents, what they told him or refused to talk about, how he learned to see things in terms of "the prison code", etc. In a later study by David SUDNOW, *Ways of the hand: the organization of improvised conduct* (1978, also 2001), the researcher takes central stage, in fact he is almost the only one on the stage as the study deals with the author's learning to improvise jazz at the piano. This is a perfect example of the second strategy discussed before, that of "becoming the phenomenon". But still the overall tone of the report is quite detached. It is the learning process that is in focus, not the piano playing person.

6.3 Two exceptional studies

I think it is fair to say that ethnomethodological studies tend to be quite detached. When the researcher figures at all in the text, his or her observations and experiences are used to gain access to the phenomena of interest, but not in terms of a personal involvement. There are exceptions, however. A first case can be found in David GOODE's (1994) book *A world without words: the social construction of children born deaf and blind*. He reports on two studies

[15] Within CA this is often expressed in terms of an "unmotivated looking" at data (cf. TEN HAVE 1999, pp.102-103).

of the life of severely disabled children which involved him in a period of very intense interaction with these girls. He is not afraid to write about his emotions, even analysing his dreams, but still these reports can be seen as providing evidence for the problems and possibilities of "achieving human understanding". In his efforts to gain an understanding of the rather closed and individual life-world of one of these children, he even tried to simulate her limitations for himself by using wax ear stops and gauzed his left eye with a single layer of lightweight gauze to simulate the scar tissue that covers the girl's left eye (pp. 33-34). By mimicking her actions under these conditions, he could gain some understanding of what she got out of seemingly bizarre repetitive movements. In a way he tried to overcome the limitations of his ordinary membership, for which she lacked the physical requirements. He also played with her a lot, in which he let her lead him, and involve him in her world. He contrasts these "free" interactions, and the possibilities for understanding which they provide, with the knowledge about such children which is produced in test situations and clinical encounters, which is extremely limited and therefore "unjust". In these ways he is much more "involved", in the sense of *engagé*, than is usual in ethnomethodology.

Another exceptional report is provided by Albert ROBILLARD's (1999) book *Meaning of a Disability: The Lived Experience of Paralysis.* As the blurb has it: "When ethnomethodologist Albert ROBILLARD began to suffer the symptoms of motor-neuron disease, he realized he was a living laboratory for revealing the countless taken-for-granted methods people use to weave their lives together." ROBILLARD's medical condition not only requires many hours of specialised care each day, but it also leaves him almost motionless and speechless in his (wheel) chair, depending intensely on those around him. He can only communicate by using a self-devised "system" of rudimentary head and lip movements to indicate letters. These have to be read one-by-one by a trained interpreter, who then can voice or type the words so formed. It goes without saying that this process is very time consuming and requires immense patience from all concerned, the author himself, his interpreters and his interaction partners. All too many people drop out of this, avoiding eye-contact and casual chatting, or reverting to a kind of staccato questioning and guessing game. Again, this report is not a detached one, but deeply personal. One of the major emotions that is expressed in its pages is anger. What raises his anger is that so often he is left out of the living tissue of everyday interaction, but at the same time, he holds up a grim mirror showing how we shut off those who can not follow our pace.

One of the aspects that these two reports have in common is that they raise moral issues concerning the membership of those persons who lack the ordinary physical capacities for participation in intersubjective communication. The social result of such communicative disabilities is mixed. On occasion, it may lead others to try to reach these people in ingenious and time-consuming

ways, but more often it means that they suffer from actual denials of co-membership. GOODE contrasts his still limited possibilities to "share" some of the experiences of the girl in and through extensive play sessions, with what he calls the "animal treatment", consisting of positive/negative reward training, that she often received from staff personnel. ROBILLARD reports many occasions in which he was effectively excluded by being denied participation in interaction, heads turning away, being pushed around without consultation, being talked about in his presence, etc. In these studies, then, the rather special membership conditions of the researchers vis à vis the research subject – through virtual participation in GOODE's case and actual identity for ROBILLARD – raised the topic of membership in an especially poignant way.

6.4 Studying specialised competencies

As noted before, recent ethnomethodological research is quite often focussed on the capacities needed for rather special "trades" such as advanced mathematics (LIVINGSTON 1986), laboratory science (LYNCH, LIVINGSTONE & GARFINKEL 1983; LYNCH 1985) or law practice (TRAVERS & MANZO 1997). GARFINKEL has in the 1970s urged his students to get specialised training in one of these trades in order to study from the inside how their practitioners created their particular version of a social "order" – which he spelled with an asterisk, as "order*" to indicate that he used it as "a collector and a proxy for any and every topic of logic, meaning, method, reason, and order" (GARFINKEL & WIEDER 1992, note 1, p. 202). It is in this context that he has formulated the "unique adequacy requirement of methods":

> "[...] the unique adequacy requirement of methods is identical with the requirement that for the analyst to recognize, or identify, or follow the development of, or describe phenomena of order* in local production of coherent detail the analyst must be *vulgarly* competent in the local production and reflexively natural accountability of the phenomenon of order he is 'studying.'" (GARFINKEL & WIEDER 1992, p. 182)

In other words, GARFINKEL has admonished his students to acquire specific membership competencies, in order to gain access to the competences that are actually used in specialised local practices.

7. Discussion

So what we have seen is that across the range of ethnomethodological and conversation analytic studies, a researcher's claimed membership of some collectivity is always at issue. In CA, researchers mainly rely on their general membership competencies – referring to "mastery of natural language" – in order to understand the interactions they are studying. In the exceptional studies of

GOODE and ROBILLARD, on the other hand, the membership they used and studied could be located on a deeper, "existential" level, i.e. "being a person" and being treated as such. Finally, recent ethnomethodological studies of rather specialised trades still requires the active use of membership, neither the general "mastery of natural language" nor the general existential one, but requiring extensive training. "Subjectivity" in ethnomethodological research, then, is a non-issue when conceived of in conventional terms, but a core issue when respecified as being a member, practically competent in the lived order* being studied.

So what about "reflexivity"? As suggested before, "reflexivity basically just denotes an object's relation to itself", but it has been used in a variety of special senses in various recent programs for the human and social sciences (cf. LYNCH 2000 for an inventory). In most of these, an element of inescapable relativism is combined with an obligation of self-consciousness. On the one hand, the suggestion is offered that since any research's results are dependent on researchers' subjectivities and "standpoints" objective knowledge is neither possible nor desirable. And on the other hand, researchers are admonished to be clear about the impact of their subjectivity and standpoint on the knowledge they produce. This may even lead to a kind of proud evocation of subjectivity, or at least the presentation of the research as a respectful dialogue of subjectivities.

Most ethnomethodologists seem to want to have nothing to do with such subjectivistic heroism.[16] For them, ethnomethodological reflexivity – if used at all – refers to an inescapable property of accountable actions, in line with GARFINKEL's definitions explicated above. Reflexivity, in that sense, is not a matter of choice, let alone obligation, but a property of human life that is constitutive of the possibility of ethnomethodological studies. As MACBETH notes:

> "Although ethnomethodological studies are not a single program [...], they tend to show a common interest in describing the constitutive practices and order-productive work of familiar, competent worlds. Rather than proposing a realist or relativist program, they tend to be nonskepticist and have no quarrels with the natives as to whether they could know what they are up to [...]." (MACBETH 2001, p. 60, n. 21)

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[16] But see POLLNER (1991) for an exception, and LYNCH (2000) for a critical discussion of his position, also MACBETH (2001).

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